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Observations on the Yellow Fever. *By* JOHN STEVENS, *Esq. of Hoboken, New-Jersey.*

(Continued from page 83.)

IN my investigations on this very interesting subject, I have travelled forward, for a considerable part of the way, in the same road with my opponents. The advocates for domestic origin, all except Mr. Webster, agree with me that yellow fever is not specifically infectious, and thence infer that it is incapable of being imported. But the fact of its having, in numerous instances, been imported, is incontestable. Mr. Webster, in his history, has recorded various instances where the proofs of its introduction from foreign sources are unequivocal. He acknowledges, without the least hesitation, that the disease was imported from Philadelphia, in the year 1798, into Wilmington, Marcus-Hook, Chester, Newcastle, Duck-creek, &c. He tells us that "this pestilential fever carried off fifty-seven persons in the village of Marcus-Hook, where the first persons seized were a shallop-man, and others from Philadelphia. But many cases occurred which could be traced to no infection. See Dr. Sayre's letter in Currie's Memoirs, p. 136."

In the enumeration of particular instances given above of importation, I have confined myself to those only where the facts are not disputed. A single instance, indeed, of its introduction into a place from a foreign source is amply sufficient to overturn the general position of the disease being incapable of importation because it is not specifically infectious. True philosophy will not tolerate the assumption of a general principle in direct contravention of an established fact. In the above instances, the fact of the disease being imported and propagated is admited. But if yellow fever is not specifically infectious, how is this phenomenon to be accounted for? I hope I shall not incur the imputation of arrogance in attempting to establish a theory, which, in my humble opinion, furnishes a complete and satisfactory solution of the problem. I have already, I trust, proved sufficiently that yellow fever belongs to a numerous family of diseases, which are the legitimate offspring of noxious combinations taking place in the atmosphere, and hence I have ventured to denominate them *atmospheric* infections.

It is a well-known fact, that all vegetable and animal substances, in favourable circumstances, undergo dissolution, and the materials re-uniting form new compounds. To this very extensive process in nature, chemists have given the general term fermentation. This process has again been subdivided into vinous, acetous, and putrefactive fermentations. To the maintenance of this process, throughout its different stages, a due proportion of air, water, and heat appear to be essentially necessary. The combinations thus formed on a great scale in nature's work-shops are infinite. There is one curious fact, however, attending this fermentative process, on which the theory I am now about to propose is dependent. It is a subject of familiar occurrence and observation, that to commence

this process a mere assemblage of the materials, in due proportions and in all other respects duly circumstanced, is not, in many cases, alone sufficient. To insure success the addition of a certain portion of materials, in which a process of fermentation of the peculiar nature we wish to promote is actually in operation, appears to be necessary. Here, then, the whole mystery of atmospheric infections is, I apprehend, at once developed. We are now possessed of the master key wherewith to unlock all the secret recesses, in which the arcana of infections have hitherto been deposited. The familiar process, of which house-wives are in daily use, will afford a satisfactory explanation of the manner in which atmospheric infections extend their sphere of action. There cannot, I think, remain a shadow of doubt, that all atmospheric infections are the result of fermentative processes. The modus operandi and the laws by which fermentative processes are regulated apply strictly to the generation of atmospheric infections. "A little leaven leaveneth a whole lump." A single spoiled herring will in a short time communicate its taint to a whole cask of sound ones: and thus, too, by a tainted ferment, have eggs been successfully inoculated.

But to illustrate this doctrine, and to apply it more immediately to the subject matter of this essay, I will introduce a remarkable instance, among many others which might be mentioned, to prove that excretory matter, imbibed by articles of clothing operates as a ferment to communicate the infection of yellow fever. In the year ——, Gardiner Baker, keeper of the museum in New York, died of the yellow fever, and his wife removed into some interior part of New England. On opening a trunk or box containing his wearing apparel, a twelvemonth after, she was overpowered by a pestiferous

smell which issued from it. In a few days after, she sickened and died, with every symptom of yellow fever.

This single case is alone sufficient to establish beyond contradiction the theory I now advance, that atmospheric infections are the result of fermentative processes. The excretory and perspirable matter exuding from the pores of the body of a diseased person, are incapable of communicating the infection of yellow fever, plague, jail fever, &c. But place this matter in circumstances favourable to the commencement and developement of a fermentative process, and a noxious compound is soon produced, which does not fail to communicate its pestilential virus to the constitution.

Mr. Webster treats as a vulgar prejudice the idea of a "single lock of cotton enkindling a flame of disease that shall desolate a city." "It is against reason, it is impossible that an affected person or two from a ship, should, within a day or two, contaminate the air of a city, and spread the principle of disease over half a mile or a mile in extent: which must be the fact if that principle is introduced from abroad."

To those who are the least conversant with chemistry, it will not be difficult to conceive how a small portion of ferment may, under circumstances favourable to the communication and reception of this specific taint, spread infection rapidly over a wide extent. For, let us suppose the atmosphere to be fully saturated with filthy exhalations arising from all kinds of vegetable and animal substances, and under the most favourable circumstances, with respect to heat and moisture, to this mass of materials the pestilential ferment is added, is Mr. Webster competent to calculate the precise velocity with which this active agent operates? In short, if the principle is admitted,

there will be no difficulty in accounting for the rapidity with which the infection of yellow fever is disseminated. When Mr. Webster condemns as a vulgar prejudice the idea of a "single lock of cotton enkindling a flame of disease that shall desolate a city," he does not sufficiently advert to the amazing tenuity of which matter is capable. Thus, for instance, a farthing candle will disseminate rays of light to the extent of a mile at least in all directions, which rays falling at this distance on the retina of the eye will affect the optic nerve so as to cause a sensation and perception of the object. Now, were it indeed practicable, to compare the quantity of matter producing this sensible effect at this great distance, with the quantity of infectious matter contained in the lock of cotton, there is no question that the preponderance would be vastly in favour of the lock of cotton. But, to illustrate this in a manner more analogous to our present subject, we will suppose this lock of cotton to be completely saturated with the infection of small-pox. It is well known that the smallest fibre is sufficient to communicate the disease. I ask, then, how many persons would this lock be capable of infecting? But here we have the matter of infection in a tangible, visible form, as it were in gross. But, how vastly more attenuated would it be in a gaseous form! In short, if the principle I am now endeavouring to establish, that infection may be communicated to the atmosphere by pestilential ferments, be once admitted, we shall be at no loss to account for this wonderful phenomenon of a whole city being infected by a single lock of cotton. Indeed, from the almost infinite divisibility of which matter is capable, we might reasonably expect still more extensive effects.

A single subject in the confluent small-pox would probably furnish matter sufficient to infect millions. Here, however,

the ferment exerts its energies within an organized system of animated matter. But, why should not matter of an equally pestilential nature, be equally productive when its energies are exerted on animal and vegetable substances, existing in a gaseous state in the atmosphere?

Indeed, specific and atmospheric infections appear to be governed by similar laws in respect to their generation and propagation. Thus, in the small pox, for instance, which is a specific infection, the most minute particle of infectious matter, when communicated from a diseased to a healthy person, will, in the body of the latter, provided it be in a situation favourable to the process, generate and multiply similar particles to an astonishing extent: and thus, too, in the case of yellow fever, which is an atmospheric infection, the minutest particle of infectious matter, when communicated to an atmosphere favourable to the process, will generate and multiply similar particles to a like extent. The only difference then, in this respect, between these species of infection, would seem to consist in this single circumstance: that specific infection is generated and multiplied in the fluids circulating in an organized system of matter; whereas, in atmospheric infections, this process is conducted in the fluids circulating in the atmosphere itself.

But, in the case above recited of Mrs. Baker, it may be said, that although it should be admitted that the disease was contracted from the articles of clothing contained in the trunk, yet it affords no evidence of pestilential ferment communicating its taint to the atmosphere. But if this case, selected from a great number of others of a similar nature which might be adduced, establishes beyond all controversy, the fact of yellow fever being communicated by infected articles of clothing, &c.,

and if, at the same time, we deny the existence of specific infection, we are compelled, without reserve, to give an unqualified assent to this conclusion: that the pestilential virus in these and similar instances was the result of fermentative processes. But from the known laws by which these processes are governed, we may safely infer, that a pestilential taint is communicable to an atmosphere duly prepared for its reception and propagation. Indeed, the history of the introduction and propagation of the disease, in the towns and villages around the city of Philadelphia, as stated above, afford irresistible evidence of the truth and justness of this inference.

But there remains one case not yet brought forward, which furnishes direct and positive evidence, establishing, so clearly, the doctrine now advanced of pestilential ferment communicating its taint to the atmosphere, that no candid non-contagionist (as usually but improperly called) can refuse his assent.

As I feel the fullest confidence that the evidence furnished by this single case must prove decisive, and determine for ever all controversy on this very interesting subject, I shall be pardoned for going into a more minute detail than I could have wished. That I may avoid even the slightest imputation of partiality, I shall exhibit Mr. Webster's statement of facts verbatim.

"The sloop Iris, capt. Truman, from Martinico, arrived (not first at Newhaven, but) at New York, the latter part of May, 1794. Her crew and passengers being in health, she was admitted as a clean vessel, and hauled up near or at a wharf. Here she lay wind bound from eight to twelve days; the precise time is not known, but it was during a long easterly storm, which clouded the sun for nine days. While at that place,

her cargo, consisting of a few salted hides, was taken out and put on board a packet, or coasting vessel, to be transported to Newhaven. During all this time, no person on board was ill; none of the vast numbers that visited the sloop received any infection, not even the persons employed in removing the hides, and transporting them to Newhaven. Early in June, but the day is not known, the sloop arrived at Newhaven, and hauled up at the end of Mr. Austin's store, within three or four rods of the back part of Isaac Gorham's house. Within a few days after which, Gorham's wife was taken ill; viz. on the 10th June, and she died on the 15th. This was the first case of the fever. Polly Gorham, who had lived in the same house about a week, sickened on the 12th, and died on the 17th, at her father's house, three quarters of a mile distant. Mr. Austin and his clerk were taken ill about the same time, and died on the 20th: Mr. Austin at New York, and his clerk at Derby, where they went on business."

To this statement I will add an account which Mr. Webster tells us is given by a respectable gentleman of Newhaven, in the following words: " On examination, it appeared, that in the beginning of June capt. Truman arrived from Martinico, in a sloop that was infected with the contagion of the yellow fever; that this vessel lay at the wharf, within a few rods of Isaac Gorham's house; that she had on board a chest of clothes which had belonged to a mariner who died of the yellow fever, and that this chest was carried into Mr. Austin's store, and opened in the presence of capt. Truman, Mr. Austin, Henry Hubbard, and Polly Gorham; the three last mentioned of whom died in a short time after their exposure to the contents of the chest. Hence it is highly probable that Mrs. Gorham caught the disease from the infected sloop or clothing. Mr. Austin's store stands within 3 or 4 rods of Isaac Gorham's

house; and no person in town was known to have the yellow fever previous to capt. Truman's arrival."

I will also add an extract of a letter written by an eminent physician and practitioner in Newhaven. "As to the supposition," says he, "with respect to local causes originating the disease, I conceive there is no occasion to seek for any other than what was contained in the chest, which was a blanket and clothing taken off the corpse of one who had died of the fever in the West Indies, and, without the least formality of cleaning, put down into a close chest, and brought to Newhaven, and lodged in Austin's store."

The facts contained in the above statements, if duly considered, without prejudice or partiality, must furnish evidence to the mind of every non-contagionist possessed of candour, amply sufficient to convince him that a pestilential taint was communicated to the atmosphere of Newhaven, by the contents of the chest above mentioned.

Without the least intimations given to the citizens by those monitory precursors, which Mr. Webster has been at so much pains to point out to us; without any previous warnings or presages, pestilence, in its most horrific form, at once presents itself, and deals destruction around. What are the thoughts which would naturally present themselves on such an occasion? It is quite inconceivable, that a disease so different in every respect from any thing that had ever happened before, could possibly be considered as aboriginal or indigenous. Mr. Webster satisfies himself, however, on this occasion, with this single remark, that "the general state of the air in the city must have been adequate to the production of the disease." But is it not truly wonderful, that this "general state

of the air" should, in this single instance, have produced an exotic which never has appeared in this place, either before or since? If we are to look to "the general state of the air" for the prevalence of yellow fever in our sea-port towns for these ten or twelve years past, how has it happened that since 1794 Newhaven has so miraculously escaped? Would not a "general state of the air," capable of producing this disease then, have been capable of producing a like effect in this city since that time, as well as in other cities? It cannot be said, that the effects of this "general state of the air" were, upon this occasion, too weak to warrant an expectation of a recurrence; for this assertion would not comport in the least with the fact. We are informed, from the most respectable authority, that the mortality in Newhaven, in proportion to its population, was fully equal to what has occurred in any other place within the union.

Since the first settlement of this country by Europeans to the present moment, Newhaven has never been visited by yellow fever, except in the instance now mentioned. In this solitary instance how is the occurrence of this disease to be satisfactorily accounted for? To have recourse to a "general state of the air," which manifested its effects nowhere else, and in this place, on no other occasion, is egregious trifling.

But, as if apprehensive that this general cause should not answer his purpose, Mr. Webster has brought forward a specific one. He informs us that there was an accumulation of stinking fish at or near the wharf the vessel lay at. But independently of the general answer, which cannot perhaps be repeated too often, viz. that mere animal and vegetable putrefactions cannot be resorted to as adequate causes of yellow fever, I would beg leave to ask how it happens that stinking fish

should, in this single instance, not only generate a new disease in the immediate vicinity of this wharf, but should be also capable of communicating a pestilential taint to the atmosphere for a considerable distance all around? For, if stinking fish alone can generate yellow fever, and spread wide its infection on all sides, with what consistency can we deny a similar effect to the contents of the chest above mentioned?

Out of a vessel arriving from the West Indies, a chest is deposited in the store of Mr. Austin, containing the clothes and blanket of a seaman who had died there of yellow fever; of four persons present at the opening of this chest, three are taken ill of the disease and die; and, in a short time after, the disease becomes epidemic throughout the city. No man, whose mind had not been previously warped by prejudice, would hesitate a moment, taking all circumstances into consideration, to acknowledge it as his opinion, that this disease originated from these infected clothes.

That it did not originate in any local cause, one circumstance attending the rise and progress of the disease in this instance, is in my mind conclusive. The four first cases that occurred were by far the most aggravated and malignant, as was manifested by the death of all the subjects of them. Now, nad the disease derived its origin from any local cause or causes, would not this have been indicated by slow and gradual approaches? Would not this higher grade of bilious fever have been preceded first by intermitting fever, succeeded by remitting fever and the ordinary bilious fever, and then by sporadic cases of yellow fever? The advocates of domestic origin, if they do not abandon entirely all attention to cause and effect, are, from the nature of their doctrine, compelled to acknowledge this to be the natural progress of the disease. But what

renders the circumstances attending this instance still more striking, is, that after the first few cases, evidently resulting from the original virus, the disease seems to have disappeared, and, when it again revived, assumed a milder aspect.

There is no possible way of accounting for these circumstances, but by the admission of the real fact, that the original virus by which the disease was introduced, was much more malignant than the taint which was hence communicated to the atmosphere, and produced the epidemic.

In this instance, I conceive, the advocates of domestic origin are completely foiled. No dexterity or ingenuity, on their part, can enable them to parry the deadly blow now aimed at their favourite doctrine. To resort to "the general state of the air," or to putrid fish, will avail them nothing. The causes assigned are in no degree adequate to the effects.

History of a Case of Hydrocephalus, and its Favourable Termination. By Dr. John Stevenson.

Newtown, Worcester County, Maryland,
April 18th, 1809.

Sir,

As you were pleased to remind me, in a note on my last communication, that "success, in a work of this nature, requires the joint operation of subscription and communication;" I now inform you, in order to convince you of the sincerity of "my wishing it success," that I have become a subscriber some time since; and now send you the following for publica-

tion, if you judge it worthy a place; together with a promise of communicating whatever falls within the small circle of my observation, that I deem interesting to the medical world.

I am, Sir,

Your friend, unfeignedly,

JOHN STEVENSON.

Dr. J. R. Coxe.

ELIZABETH L——, aged nearly two years, was seized, on the 7th of August, 1808, with the usual symptoms of cholera infantum. Nothing uncommon appeared for the first day, and the mother being sick at the same time, I was called at 11 o'clock on the morning of the 8th. After prescribing for the mother, I was informed of the child's illness. On examination, it appeared to have but little fever, and gave me a pleasant smile when I uncovered its face. The family informed me it had been extremely ill the preceding day and night, with a vomiting and purging, but had been better all the morning. I observed, that as the mother had just taken an emetic, she would necessarily require all the attention the family, at that time, could bestow; and as the child was better, I should defer giving it any medicine until my return, which would be in about two hours.

About an hour after my departure, an exacerbation of fever came on, and the child had violent convulsions.

On my return, I found the room crowded with attendants, who, seemingly in a fit of desperation, had given a dose of laudanum, and immersed the child in warm water.

The fits were in a measure relieved, though they continued, lightly, through the afternoon, with longer intervals, and some permanent stiffness of all the extremities.

I was apprehensive the fits were occasioned by some acrimony in the primæ viæ that remained undischarged, and gave as a purge 3 grs. of calomel*, mixed in syrup, and directed a large tea-spoon full of ol. ricin. to be taken every two hours, until it should purge freely. Shortly after night, the fever seemed to increase, and, together with it, the convulsions returned at shorter intervals, and, lasted longer; the abdomen hard and tumefied, and no evacuation had taken place from the bowels. I grew extremely solicitous for the fate of my patient, and now made an attempt, and succeeded, in obtaining about five ounces of blood from the arm. A strong stimulating injection was thrown in the rectum, which was shortly returned without any fæces; at length, however, a stool was procured, followed by two more in the course of the night, and considerable relief was obtained. There still remained some stiffness and a spasmodic contraction of the arms, so that it was seldom any accurate knowledge could be obtained of the pulse by feeling it at the wrist; I was, however, generally satisfied when I examined the feet.

The child remained tolerably quiet the remaining part of the night. Epispastics were applied to the extremities, which, by the morning of the 9th, were well drawn.

* I had lately been led to adopt an opinion, from actual observation, that calomel, in an *appropriate dose*, would operate more speedily as a cathartic than in a *large one*. I mentioned the circumstance to a medical friend, thinking I had discovered something not generally known. He informed me he had long ago observed the same.

On my visit this morning, the dilated pupils of her eyes were the first thing that I noticed. I immediately mistrusted a diseased state of the brain; and, in order to produce a derivation from the head, gave 2 grs. cal. and 5 of jalap, to be repeated every four hours until it should produce copious evacuations from the bowels; but so insensible were the intestines to the impression of the medicine, that she relapsed into fits the succeeding night, more violent than ever, before a single evacuation was obtained from the medicine. Recourse was again had to ol. ricin. and strong enemata, which again relieved her, after being, apparently, in the act of dissolution, with every muscle composing her little system most tremendously agitated.

The bowels, after this, were not so obstinately constipated; cathartics now operated in their usual dose, and were freely used; and blisters were applied, alternately, to nearly all parts of the head. The fever was now completely subdued, but the pupils still remain dilated; no strabismus has at any time been observed; she has spoken but once since she first had fits, and that apparently in a great fright: she called her papa who was near her. She continued to lie in a torpid, lethargic, insensible state, for near three weeks, with an incessant desultory motion of her arms, unaccompanied, I am free to say, with any consciousness whatever; for the MIND, strictly speaking, *was entirely obliterated.* The loudest noise would excite no impression; the handsomest toy would not produce a single effort to handle, or even look at it.

I felt rather in an awkward predicament; I never saw a case similar to the present. She was by this time much reduced; had been weaned from the breast a short time before taken down, and again put to it; would at times suck extremely

hearty; appeared to be unconscious either of the quantity taken, or its feelings of hunger; would appear equally uneasy while at the breast as any other time. In this dilemma, nothing appeared better to be done than an adherence to the golden rule, of "prescribing according to the state of the system." Accordingly tonics were most plainly indicated, and nothing seemed to promise so much as the cold-bath. I never had tried it, in a single case, therefore felt a diffidence. Electricity was out of our reach; neither did it appear so flattering. At length, after some days spent in deliberation, recourse was had to the advice of Dr. S. Ker, of the adjoining county, who recommended the bath in such terms, that it was begun with the very next day, being the 6th of September, and continued about four weeks, regularly once a day, with the most astonishing and encouraging effects.

The bath was composed of a large tub, set in the room, filled with water from the well; for the first few times made a little warmer. The child was entirely stripped, simply plunged, immediately received in a warm blanket, rubbed well with dry flannel, and wrapped warm.

It was highly pleasing to see the resuscitation of its mental faculties, and the gradual manner in which they were re-evolved. The bath was used about eight days before any effect was perceptible. The first effect observed was (what would most naturally be expected) signs of uneasiness on being stripped; some days afterwards she would look at the tub before being stripped. She, after a while, would follow with her eyes the other children about the room; and a while, still longer, afterwards, began to attempt to support herself on her feet (for I ought to have observed before, that her feet have been as useless as her organs of speech, and have

been idle as long), and finally she made an effort to speak, for the first time, wanting two days of seven weeks.

The exhibition of the martial tincture was begun at the same time the bath was used; but, having some constipating effect on the bowels, and the appetite sufficiently good, it was not long continued. The bath was intended to have been continued somewhat longer, but an eruption (the psora) appeared generally over the skin, with such intolerable itching, that the bath was laid aside for frictions of sulphur. The child, however, continued to mend rapidly without any further use of it.

I have not observed her speech, or the use of her lower extremities, to improve any faster, in consequence of her having had a previous use of them, than is customary in other children.

She is now, though rather small of her age, in perfect health, lively, and active.

Experiments and Observations on the raising of Wheat Flour, and Buckwheat Meal. By JAMES WOODHOUSE, *M. D. Professor of Chemistry in the University of Pennsylvania, &c.*

AN opinion has been adopted by many philosophers in Europe and America, that the raising of bread is merely owing to a discharge of carbonic acid gas or fixed air; and it has been asserted, that there is no difference between the properties of flour, and bread when it has been baked.

This opinion was supported by the late Dr. John Penington, in a volume of Chemical and Economical Essays, in the year 1790; and, since this work has been published, this theory has generally prevailed in the United States. The editors of the Encyclopædia Britannica brought forward what they supposed to be a few facts in support of it, and they supposed that common atmospheric air, entangled in the midst of dough, would be sufficient to raise it.

The principal argument in support of this opinion is, that all waters which contain carbonic acid, such as those of the Saratoga and Ballstown springs, in the state of New York, easily raise bread.

It is not my intention to prove that the raising of dough is not owing to an escape of fixed air, but to show,

First, that flour mixed with water, however strongly impregnated with carbonic acid, will not make light bread.

Secondly, that the best bread can be made without any fixed air; and

Thirdly, that a chemical change takes place in the component parts of flour when it is made into bread, and which arises from the decomposition of water.

A quantity of water, impregnated with carbonic acid, by strong compression, was accurately mixed with buckwheat meal, and exposed to a proper temperature. It exhibited no signs of rising; the meal subsided into a heavy mass, and the water floated over its surface.

A quantity of yeast was filtered. A viscid residuum remained on the filter, which contained no carbonic acid.

Half a tea-spoon full of this substance, triturated with warm water, and well mixed with buckwheat meal, raised it completely in four hours.

Dough was made from wheat flour, by well mixing it with water containing carbonic acid.

It was also made with the viscid residuum and water.

The masses were placed along side of each other by the fire, and, in the space of a few hours, that made with the residuum was well raised, but the mass from the carbonic acid exhibited no change; and when the two were baked, the loaf made without the fixed air was extremely light and spongy, whereas the other was tough and heavy.

These experiments clearly prove, that buckwheat meal may be raised, and excellent wheat bread made, without any fixed air; and that water, however strongly impregnated with carbonic acid, and mixed with flour, will not afford good bread.

The bakers of Paris make this article of an excellent quality, yet all their yeast is brought in bags, in a dry state, from Flanders and Picardy.

Good yeast contains a large quantity of fixed air; but so little of it is used in raising buckwheat meal and wheat dough, that it cannot have much effect in raising these substances.

By boiling forty cubic inches of yeast in a glass retort, the mouth of which communicated with a receiver filled with mercury, fifty-two cubic inches of fixed air were obtained.

A small loaf of bread was raised with the viscid residuum, in one hundred and four cubic inches of atmospheric air, confined by water. In the space of sixteen hours, the air was found to contain forty cubic inches of carbonic acid gas. Upon washing away the fixed air, and testing the atmospheric air by phosphorus, it was found to have undergone no alteration.

Buckwheat meal was raised in the same manner, and with the like effect.

These experiments prove, that a chemical change takes place in the component parts of flour, when it is made into bread. No carbonic acid was contained in the dough when it was first made, and yet, in a short time, forty cubic inches of this gas was formed.

Fixed air is composed of carbon and oxygen.

As no oxygen is contained in the flour, and no carbon in the water, the oxygen of the water must combine with the carbon of the flour, and generate the air which raises the bread.

It is a difficult matter to say what becomes of the hydrogen of the decomposed water. It does not unite with another portion of oxygen and carbon to form alkohol, for none of this fluid can be procured by distilling dough or buckwheat meal, after they have been raised.

An Essay on the Treatment of Oblique Fractures of the Os Femoris. Read for junior membership, by EDWARD M. FOULKES, *before the Philadelphia Medical Society, January*, 1809.

SIR,

AFTER the repeated solicitations of many of my acquaintances for the publication of the following essay, I have at length consented to submit it to your judgment; and if you think it worthy of promulgation, you are at liberty to insert it in your useful Museum.

I am, dear Sir,
With sentiments of the highest esteem,
Your humble servant,
EDWD. M. FOULKES.

Dr. J. R. Coxe.

MAN, from the love of fame, or from motives purely charitable, has been busily engaged, for centuries past, in inventing, devising, and executing means for the relief of his suffering kind. To prove this, it is necessary only to advert but for a moment to the records of the science of medicine. Contained there, we shall find an account of the successive improvements in that science, not less astonishing than pleasing. It is astonishing, because of the discovery of the wonderfully seeking, and in most instances *finding*, ingenuity of man. It is pleasing, because of the discovery of the means being efficient in the relief of suffering mankind.

When charitable motives give rise to such attempts, men, so far from becoming the objects of derision and ridicule (as

ought to be the case when caused by a desire of fame) are, in the estimation of the learned, the objects of incalculable affection.

I then shall offer, as an apology for the following innovation, the action of these motives.

It is a subject of no little surprise, that, considering the length of time which has elapsed since the earliest period at which surgery was practised, and the many accidents and diseases to which our ancestors were exposed, during this time, belonging to the province of surgery, there should be so many desiderata in this branch of the healing art. Thus, in oblique fractures of the os femoris, it was reserved till the late period at which Dessault lived, to discover the method of obviating (by means of permanent extension) that, that the more ancient surgeons thought to be a necessary consequence of such affections. The reader will readily perceive, that I mean deformity and shortening of the limb. And these are, even now, not prevented but at the expence of great suffering to the patient, notwithstanding the improvements made by doctors Physick and Hutchinson upon the treatment recommended by the French surgeon.

Before proceeding farther in this essay, it will be proper to mention more particularly why it was undertaken, and the order to be observed in it. It was undertaken, in common with all other means, because of the hazardous, helpless, and agonizing state of persons labouring under those affections, before means for their relief were had recourse to. Secondly, because of a knowledge of the sufferings of patients while under both the ancient and modern treatments. And, thirdly, from seeing frequent failures in the cure as attempted by those means. The

first thing, then, which shall occupy a part of this essay, will be a mention of the objections to the most successful ancient and modern treatments: the second, a statement of the indications of cure: the third, a description of the means and the method of applying them, by which I think the indications will be fulfilled: the fourth, an inquiry to ascertain whether or not they are: the fifth, an attempt to do away all objections to it: and the sixth, a brief detail of its advantages.

The objections to Dessault's apparatus for making permanent extension are, first, the liability of the parts on which the roller for making counter-extension acts to be excoriated, by the roller becoming somewhat like a rope, and consequently bearing on but few points with great force.

Secondly, by the oblique passage of the roller over the superior part of the thigh, the muscles on the internal side are irritated, and in consequence contract, producing thereby derangement both in the length and direction of the thigh; in which last it is assisted by its own oblique pressure on the superior fragment, tending to carry it upward and outward.

The muscles most exposed to the irritation, and by whose contraction this effect is produced, are, the adductor magnus femoris gracilis, or rectus internus, sartorius, semi-tendinosus, and semi-membranosus.

For these muscles, originating from the ramus pubis, and tuberosity of the ischium, and inserted into the inner and inferior part of the femur, and into the inner and superior part of the tibia, their action certainly would have the effect of drawing these parts upward and inward; and in proportion as they were drawn in this latter direction, would be the displacement

of the fractured portions with regard to the direction of the thigh. For the muscles which are inserted into the lower end of the superior fragment are so deep seated as to be without the reach of the irritating power of the roller, and of course do not contract so as to keep that end in contact with the other, and farther prevent its being carried outward by the pressure of the roller. It would then form an angle with the superior end of the inferior fragment, whose point would be outward, and the angle looking inward, so than the knee would be much nearer the sound leg than would be consistent with proper walking and the good figure of the person.

This second objection applies with much more force in fractures of the neck of the bone, than of its body.

In the modification of the external splint of Dessault, by Dr. Physick, these objections apply with much less force. For, as regards the first, the danger of excoriation in the perineum is lessened in proportion as the pressure on it is; which is done by dividing it between the perineum and axilla; and also in proportion as the roller is carried from an oblique situation, to one more in the direction of the thigh.

And as regards the second objection, both the danger of carrying the superior fragment outward, by the pressure of the counter extending roller, and the irritation of the muscles by its oblique action, are much lessened by the passage of the roller in a direction approaching nearer that of the thigh. Nay, the doctor even tells us how we may make it act precisely in this direction.

But as I shall presently say this method is likely not to be employed, because of the ill conveniences attending it, doing away the propriety of its application.

I come next to point out the objections to the doctor's manner of treatment. They are, first, as in Dessault's method, an inefficiency in preventing a shortening of the limb and deformity in its direction, produced by the action of the counter-extending roller irritating the muscles, and carrying the superior fragment outward, as was explained when speaking of this as applying to Dessault's apparatus.

But, say the votaries of Dr. Physick, we are taught to avoid this, by tying a bandage to the counter-extending roller on its anterior part, and passing it around the unaffected side of the body, and tying it tight to the posterior half of the aforesaid roller; by which we pull it from the affected side, in a line running perpendicularly from the tuberosity of the ischium over the groin, and up about an inch and a half distant from and parallel to the linea alba. This brings me to the consideration of the second objection, and which I formerly hinted at: viz. the inconveniences of the continued application of the transverse roller; or that which is used to pull the counter-extending roller in the aforementioned position.

It must be obvious to all persons, that the force necessary to be employed for this purpose must be very considerable, and, to do this, it acts on the unaffected side of the thorax; from which three ill consequences are apt to arise. Before proceeding to mention these, I beg leave to observe once for all, lest, from a neglect of it, I may not be perfectly intelligible, that when I speak of the unaffected side of the thorax, the reader is to understand that side which is opposed to the fractured thigh, and the reverse when speaking of the affected side, that is, the side corresponding to the diseased thigh.

The bad effects of this roller are, first, a difficulty of confining it in a horizontal situation, and which is necessary; for if the part of the roller passing on the unaffected side of the thorax be left unconfined, it will either ascend or descend, because of the conic figure, and motions of the chest by respiration.

Now, if that part of the roller descends, it will pull its ends down, which, approaching to the unaffected side, by reason of the obliquity of the counter-extending roller, will entirely destroy its action: for it is in this situation perfectly relaxed; and the consequence of this is, a resuming, by the counter-extending roller, its first oblique position and action.

I am aware that it will be said, in reply to this objection, that the transverse, instead of being tied to the counter-extending roller, should be sewed to it, and that by so doing the consequences of its slipping down would be avoided. But even this would be attended with hurtful consequences: for, in proportion as the roller moved from a horizontal situation to an oblique one, would be the consequent pressure, and the danger of excoriation. Nor is this all, for the ascending and descending parts of the colon, lying between the small intestines and parietes of the abdomen, would be, according as the right or left sides of the body are acted on by the descended roller, very much compressed; and consequent, and dependent on this, would be an imperfect or an entire cessation of the performance of the functions of that intestine.

Should the part of the roller lying on the unaffected side ascend, its ends, whether tied or sewed, cannot follow it: for, when tied, the counter-extending roller goes so obliquely outward from the place where the two are attached to each other,

that it does away all possibility of such an occurence; and, if sewed, it is evident, the ends could not move.

Now the consequence of this would be, augmenting that, that before was too great, to wit, a difficulty of respiring freely, caused by pressure. For, as I before observed, in proportion as the roller moved from a horizontal to an oblique situation, would be the consequent pressure. This impediment to respiration being afforded when the roller remains in its place of first application, is the second objection to its use.

The third is, from its great pressure, it is very apt to produce excoriation of the parts on which it acts.

The next objection to Dr. Physick's apparatus is, that the external splint presses in the axilla: from which results four ill consequences, namely, in the first place, the circulation in the axillary artery is almost completely stopped, and the return of the blood is also stopped. Arising from this are, first, a numbness and disagreeable pricking in the arm, forearm, and hand; secondly, discoloration of the afore-mentioned parts; thirdly, coldnesss of them; fourthly, swelling; fifthly, vesications; and, sixthly, mortification of the parts.

Any person may satisfy himself of the truth of the first, second, and third remarks, by sitting thirty or forty minutes in a chair whose back extends up as high as the axilla, with the arm thrown or hanging over it, in such way as to allow the weight of the arm to be supported by the bearing of the axilla on the chair. He will, as before said, first feel a numbness and pricking sensation in the depending parts, secondly, a discoloration of a bluish red will come on, and, thirdly, cold-

ness. If now the radial artery be felt, its accustomed pulsatory motion will be found to have deserted it. By a continuance in this position even one week, the accession of all the before-mentioned horrible effects will take place.

The second inconvenience is the extreme danger of producing excoriation where the splint touches the pectoralis major and latissimus dorsi.

The third is, either a painful swelling of the axillary glands, which is apt to terminate in cancer, or such a degree of inflammation as to be troublesome, and to require a pretermission of the use of the splint.

And the fourth ill effect resulting from the use of the splint is, that it pushes the shoulder so far from the spine of the ilium of the affected side, as, by putting the muscles and integuments completely to the stretch, to be productive of the severest pain and greatest distress to the patient.

I once asked the adjunct professor* of surgery in the University of Pennsylvania, whether or not he had seen the aforesaid effects accrue from Dr. Physick's method of cure? (For I had not seen a case of fracture treated by the doctor's method, and of course could not ascertain it from experience.) His answer was, that he had not. I then asked him, if he did not think their occurrence quite probable? he said no; and gave this as a reason, more, though, to account for their *non-occurrence*, than to answer my last question. It was this, that the patient might alternately support the pressure on the perineum by the counter-extending roller, and in the axilla by the splint,

* Dr. Dorsey.

and thereby avoid those bad effects. But it must be obvious, that if the patient tried to free himself from the pressure of the splint, he would augment the pain and distress already existing, in consequence of the stretching of the muscles and teguments of the side, for he would have to bend his body to the unaffected side much more than the splint could possibly do, and in proportion as this is done will be the increase of pain. So, with due deference to Dr. Dorsey, I do not think that he has satisfactorily accounted for the non-occurrence of those effects.

Then how shall we explain the fact? We can explain it by saying, that the splint does not press in the axilla so as to take off half of the counter-extending pressure from the perineum. And this I believe to be the true cause. And I farther believe, that if the splint were three inches shorter, cures would be effected with the same ease and perfection that they now are. To substantiate my belief, look at the experiment with the chair, consider the exposure of the blood-vessels, glands, &c. the tenderness of the parts, the nature and continuance of the pressure. A due consideration of these things cannot but induce a belief with me, even in the most sceptical mind.

To the foregoing objections to Dr. Physick's splint may be added this, another inconvenience, viz. that all persons not of a particular height or length must have a splint made expressly for themselves. An inconvenience this, worthy of much attention.

I hope I shall not be censured for having been thus bold in decrying a treatment whose advocates are not less respectable than numerous. For it is, I believe, uniformly the case, that whenever innovation in science is attempted, the innovator

finds it necessary to decry or expose the imperfections of the old, so that the world may see that perfection was not had; and now their wishes for it prompts them to seek it, in doing which they are almost certain to give this attempted innovation an attentive examination. Should I fail in the following attempt, I hope my errors will serve as a clue, to reason from which, some more happy genius may discover that mode of treatment which shall in every respect be complete. Nor is this improbable; for, says Rush, " New truths are seldom discovered, but at the expence of old errors."

I proceed next in order to mention the indications of cure which appear not to have been fulfilled by former apparatus.

They are, first, to make the roller which passes over the tuberosity of the ischium next the affected thigh, act in this direction of the limb.

Secondly, to prevent perfectly any pressure in the axilla.

And, thirdly, to lessen the pressure on the perineum.

It being now in order, I proceed to a description of the means by which I think they are fulfilled. In the superior end of Dr. Physick's splint, I would have the crutch-like excavation so deep as not to come in contact with the axilla, and of course, when it acts, not to press there.

To each part, A A, of the splint as they pass before and behind the joint of the arm, at their tops, is to be fixed, by an immoveable joint, a piece of wood B, of the same width of the upper end of the splint, making an angle of about 135 degrees with it, and about nine inches in length. This is also to be exca-

vated, but at its lower end, and the excavation is to be so large as not to touch the shoulder which it embraces; its upper end may be rounded in the form of a shingle; and it is to have on its superior surface a piece of light wood C, about an inch thick, glued; this is to strengthen and prevent this part from splitting, for on it counter-extension is to be made. Between the edge of the excavation and the superior convex end, are to be two mortise holes D D, about an inch and a half long and half inch wide. At the immoveable joint made by the piece B and the splint, it would be well to have a piece or block of wood E, fitted and glued well, to enable the joint to support better the strain, which is considerable. The length of the splint, taken together with that of the piece of wood B, should be between six and seven feet. The splint below the middle point, between the hip and arm-pit, may be of the ordinary width, but above that it must be so wide as to admit the hole made by the two excavations to be so capacious as not to touch axilla, shoulder, nor any other part in their neighbourhood; and also so wide as to make the branches or horns strong enough to sustain the pressure of counter-extension. It (the splint) may be of the ordinary thickness. Two leather straps F and G; the one F should be twelve inches in length, and about an inch and a half wide, and have two common button holes cut in one end, while to the other a common buckle is to be sewed. The other strap G should be about three feet in length, and as wide as the buckle of the strap F; this should also have, at one end, two button holes cut, and through the greatest part of its length, beginning at the end unnoticed, should have holes punched to receive the tongue of the buckle of the strap F.

A roller should be procured of about five feet in length and two or three inches in breadth. The pantaloons of the patient

(if a male), if not, the pantaloons of a person about the size of the patient, should be ripped in the seam which unites the two legs, so as quite to separate them; to the bottom of the leg of the pantaloons belonging to the unaffected thigh, should be sewed a common sock; let it be cotton or woollen, according as the season is warm or cold in which the accident happened. A pad of chaff long enough to reach from the groin to the instep. A splint about four inches shorter than the pad, and of the common width, and a common roller to confine them. These things being had, and the patient laid on a suitable place, viz. a mattress, with the body-bandage of Dessault under him, and the splint cloth, and bandage of strips placed under the affected thigh, as directed by Dr. Physick, the leg of the pantaloons, having the sock sewed to it, is to be put on the unaffected limb and pulled up well; the strap G is then to be buttoned to the posterior button of the pantaloons, to which the suspenders were wont to be, and its other end carried to the shoulder of the affected side: extension and counter-extension are now to be made, the fragments put in apposition, the bandage of strips applied, the two short pasteboard splints, which go on the anterior and posterior parts of the thigh, and the two pads of chaff, are all to be applied according to the directions of Dr. Physick, by whom they are used and recommended; this done, the splint which I have described is to be wrapped up in the splint cloth, care being taken to leave enough of it to wrap up the internal splint of Dr. Physick. After being unfolded, the arm corresponding to the affected limb must be put through the hole or opening at the top of the splint, in such way that the holes D D will be over the middle of the shoulder; the body-bandage is now to be tied around both body and splint, so as to fix this last to the former, and also prevent its bending at the joint H; the strap G is to be put through the posterior of the holes D D, when its end will be over the superior surface of the upper end of the

splint, it is then to be put through the anterior hole and carried down over the breast. The strap F is now to be buttoned to the anterior suspender button of the pantaloons, to which then the strap G is buckled, and may be drawn as tight as the surgeon pleases. After the extension has begun by the splint, the five-feet roller is to have its middle applied to the perineum of the affected side, and its two ends, one posterior and the other anterior, are to be carried up and passed through the corresponding holes D D, and then tied to each other. The extension is to be made after Dr. Physick's directions or method. The remaining part of the treatment, as the application of Dr. Physick's internal splint, the tying the bits of tape to make the whole firm and secure, is according to the doctor's method.

There is a circumstance, which, though not properly an indication in the cure, deserves the attention of the surgeon to prevent even a possibility of its occurrence, viz. that from an inclination in the patient to change some little the position of his body, derangement is produced: for to do this he must draw up the foot, and fix the leg nearly perpendicular, and then cause the muscles on the anterior parts of the body and thigh to act so as to make the foot and head be the points on which the whole rested; these therefore must be fixed, and, as being so, may be said to be the origins of the muscles concerned in producing the wished-for yet deleterious motion. I say deleterious motion, for a sort of circumrotatory motion is given to the unaffected side, whose centre corresponds to the axis of the affected thigh; now the lower fragment not partaking of the rotatory impulse, must remain steady, and the consequence is fresh derangement. Then, to avoid the danger of this, I would have the pad formerly described, placed on the anterior parts of the thigh and leg which are unaffected; over this the splint should be applied, and the whole secured by a roller.

The apparatus being described, and its manner of application, I come next to inquire whether or not the indications of cure are complied with. The first certainly is: for a line running from the tuberosity of the ischium up to the middle of the shoulder, when the body is perfectly erect, will be found to be perpendicular or nearly so to the horizon, and this is the direction of the thigh, and precisely the situation of the roller, which has for its object the prevention of the descent of that side of the pelvis corresponding to the diseased thigh, and which tendency is given to it by the action of the leather straps and the extending roller; but in doing this (which it effectually does), it also makes half of the counter-extension.

It is equally certain that the second indication is fulfilled; for it must be remembered, that, when describing the splint, it was particularly observed, that the crutch-like excavation was to be so deep and large, as not to touch the arm or axilla.

And as to the third indication, it is not less evidently fulfilled. For half the resistance necessary to be afforded to prevent the body following the lower fragment, which is continually pulled down by the extending roller, is taken from the perineum, and transferred to well and distant parts (the foot, leg, and thigh), by the splint offering a place near the unaffected side, for a counter-extending roller to act on. Now, was this attempted in other apparatus, the obliquity of the roller would be such as to produce inconveniences so great as to require a discontinuance of its use.

It is next in order to inquire whether there be any disadvantages attendant on the continued application of this apparatus, whether these are greater than those of other apparatus, and

whether they are such as do away the propriety of its application?

There is one, but this can be obviated with the greatest ease: it is, that the bulk or size of the part A, which passes behind the shoulder, is such as to support a part of the weight of the body, for it cannot lie in perfect contact with the bed near this part. The objection is done away, first, by pressing the splint directly down on the bed after it is applied, by which a depression or groove will be made for the lodgement of the part; the body will then lie on a plane whose superficies corresponds to that of the bed. Secondly, by the imposition of a flannel pad between the shoulder and splint. And thirdly, by having two splints, one for each side, these having the posterior parts cut entirely away, while the anterior are made so strong as to support all that pressure now borne by both parts.

There is one other objection, which is, that counter-extension cannot be made on both sides, when both thighs are broken. But, if my reply to Dr. Dorsey's assertion be correct, this objection is quite futile; nay, in making use of that reply in controverting this objection, it receives additional probability of correctness, from the circumstance of the counter-extending roller acting in the direction of the thigh, and thereby employing but little force to do that, that requires the greatest when it passes obliquely, as it does in Dr. Physick's method of treatment.

To exemplify my meaning, let there be a body weighing two drachms; to this, by a cord, fix another weighing eighty-six grains; place the heaviest on a well-planed table, so near its edge that the lesser may hang an inch or two over; then re-

move the hands from them, and it will be found that the lighter body will draw the heavier entirely off.

Secondly, remove the lighter body from the cord, and in the place where the heaviest lay, in the experiment just mentioned, put it again: then lay a ruler parallel to the track observed by the body in going off the table: let the ruler be thin and smooth, measure the distance at which the body is from the edge of the table, and then measure the same along the edge, beginning at the edge of the ruler, which is coincident with the scite of the cord, as in the first experiment. After getting this by measurement, place the cord there, in such way though, that it will cross the ruler; that is, the ruler must be between the body on the table and the point on the edge of the table, at which the cord is to act in pulling the body. Now suspend to the cord a weight which will draw off the body, which will require a weight equal to two hundred and twenty grains; for it must be observed, that to do this (pull it off) it acts in the diagonal of a square, one of whose sides is the distance from the edge of the table to the body, and another from the same point to the place on the table at which the cord touches in hanging down. Here then we see that the force necessary to do the same thing, when employed in the diagonal (or, in other words, when acting obliquely), is nearly thrice as great as when acting in a line parallel to the square, that is, in a straight line. Now my reply to Dr. Dorsey's assertion was, that I believed the pressure of counter-extension to be supported solely by the perineum; but, if the perineum be able to support almost thrice the pressure necessary to keep up the superior fragment, as the above experiments show to be the case, how much more easy, then, will it support the necessary pressure? no more than which would be made, even after excluding the leather straps and pantaloons; and for this plain reason, that the counter-ex-

tending roller, in the treatment I have proposed, acts precisely in the direction of the thigh.

Besides, Dessault says, that his cures were generally perfect. Now counter-extension, in his mode of treatment, is made entirely on the perineum; and we know that his counter-extending roller acted so obliquely as to approach near a right angle; and if he could obtain cures in this way, it would be unaccountably strange that we could not obtain them with less suffering to the patient, in less time, and with more perfection: for we proceed in the cure with means accurately adapted to the indications given by the nature of the affection.

There is another objection, which is a little danger of producing tenderness and excoriation on the heel of the unaffected side. But this is so easily avoided as scarcely to deserve time to mention the means. All that is necessary is to elevate the leg, by placing a pillow under it, taking care to prevent its lower edge extending to the heel.

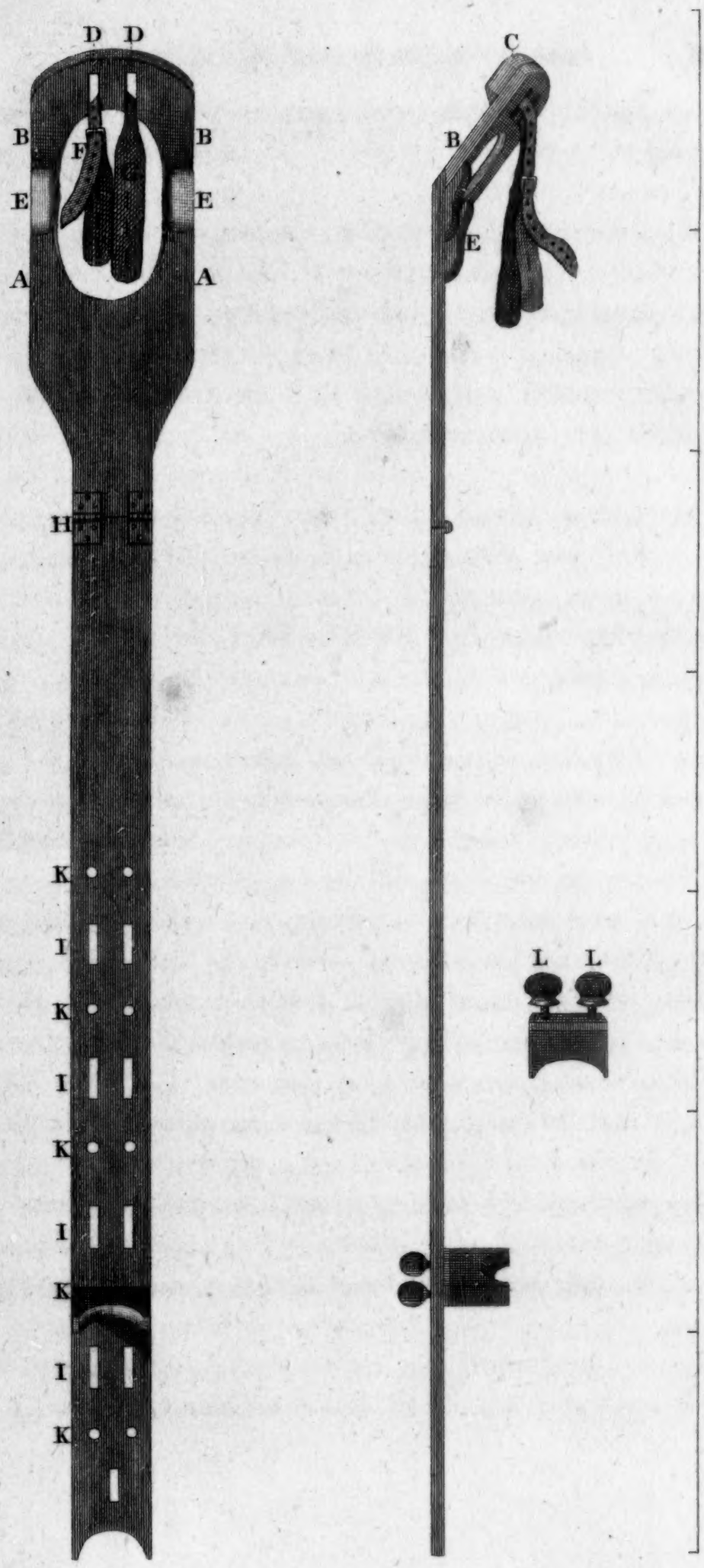
After having stated the objections to the treatment I have proposed, and endeavoured to do them away, it is left for the candid reader to judge of their weight, and of my success in refuting them; or, in other words, it is for him to determine whether or not the disadvantages attendant on the treatment, are such as to do away the propriety of its application.

It remains now for me briefly to mention the uses of some parts of the splint not yet treated of; and then conclude, by simply stating some of its most prominent advantages.

The parts not yet spoken of are the joint H, the mortise holes I I I I, the gimlet holes K K K K, and the screws L L,

which fasten the block to the splint intended to make the extending roller act in the direction of the leg. The use of the first, namely, the flexible joint, is to make the splint portable, by allowing it to be doubled up, so that the surgeon need not be at the trouble and expence of making one in every case, and which all would have to do, except those who practise in hospitals or like institutions. This would invariably be the case with a surgeon in the country; unless, indeed, the accident should happen at his own house.

The uses of the second, third, and fourth parts, namely mortise and gimlet holes, and screws, are to adapt the point of extension in the splint to the different lengths of patients above twelve years old. For, by unscrewing the screws, the block may be moved and fixed three feet up the splint, and at any intermediate point the surgeon chooses. Now it would have been inconvenient, and perhaps detrimental, to have had a long extending roller, and, after fastening it properly to the foot, carry it down to the end of the splint, and there secure it in the ordinary way, which would have to be done whenever short subjects were submitted to its operation. But the gimlet holes allow the block to be fixed as near the foot as the surgeon wishes, by their admitting the screws in them; and the mortise holes allow of the roller being fastened to the splint as soon as it has passed over the block, so that a roller of ordinary length may be used with all its advantages. The mortise holes may be about six inches apart, but disposed in pairs, that is, directly adjacent to each other; and may be an inch and a half long, and half an inch wide. The gimlet holes should be about midway between the mortise holes, and disposed also in pairs.



Engrav'd by Lawson for the Philad.ª Medical Museum.

From these contrivances it must be obvious that the objections made to Boyer's apparatus for treating those fractures are not applicable to this.

The objections were, that every patient not of a particular length was obliged to have another machine made. Here, on the contrary, as was before said, this, by simply moving the block, will fit all persons above twelve or fifteen years old. This is an advantage possessed, that but very few of the former apparatus can boast of. And now to conclude.

The advantages of the treatment proposed are, first, that half of counter-extension is made on a surface so extensive (the foot, leg, and thigh) as to escape entirely the notice of the patient, so that inconvenience from counter-extending pressure is not to be apprehended.

Secondly, the roller passing on the tuberosity of the ischium acts precisely in the direction of the thigh, without the aid of a transverse or any other hurting roller.

The consequence of this action is, an exemption of the muscles of the thigh from irritation, and thereby avoiding the danger of producing derangement in the lower fragment; moreover the danger of carrying the upper fragment outward by the presure of the roller is perfectly done away.

Thirdly, all the ill effects of pressure in the axilla are avoided.

Fourthly, the use of the arm corresponding to the diseased thigh is permitted to the patient; whereas in the mode of treatment recommended by Dr. Physick, if the splint presses in the axilla, he is deprived of it. If there were no other advantage

derived from the confinement of the sound leg, this would amply compensate for it. Who is it, that would not rather have the use of his arms when confined on his back, than of his legs?

Fifthly, as the counter-extension is made on both sides of the pelvis, this is kept square, whereby a very accurate opportunity is afforded for measuring the two limbs, that is, comparing their lengths with each other.

Sixthly, by its fulfilling so completely every indication of cure, there is reason to believe that it will not only make the cure more perfect, but even expedite it.

Seventhly, it may be employed with all the aforementioned advantages in oblique fractures of the bones of the leg, when, from excoriation of the leg by the counter-extending roller, the ordinary apparatus is to be discontinued.

And eighthly, because of its simple structure, any mechanic can make it, from having it described to him by the surgeon.

Account of a Fœtus found in the Body of a Child, with the Dissection. In a letter to the EDITOR, *from Dr.* EDWARD B. GAITHER.

Springfield, Washington County (Ken.), June 4th, 1809.

SIR,

AS the science of medicine is not yet consummate, and believing that nature has in reserve many secrets of great importance to the human family, which the mind of man is capable of grasping and comprehending, should accident or a chain of reasoning present them a clue to direct their inquiries, I should conceive myself unpardonable not to present to the medical world, believing the case not on record, a very singular phenomenon, which the learned may avail themselves of either to elicit or extort from nature many valuable secrets.

On the 7th of April, in the above-mentioned county, I was called to visit a female child, the daughter of John Milbourn. She was two years and nine months old, and was supposed to be affected with ascites. She died about three hours after my arrival. Her parents gave me a detailed account of her case, and its various symptoms. I was by no means satisfied that it was a real dropsy, though there was great tumefaction and tenseness of the abdomen, and fluctuations evidently felt when prest by the hand; but as the symptoms were some of them inappropriate to that disease, and others so equivocal, I could not but suspect that her disease had been either unknown or misconceived; I prevailed on her parents to permit an instrumental examination.

The operation was performed by a longitudinal incision passing from below the sternum and reaching near the pubes, and a transverse one passing through the epigastric region and sides; a cavity was opened to about half the distance between the abdominal cavity and the anterior surface, that discharged between three quarts and a gallon of yellow water, which smelled like rotten eggs. Within the cavity was found a monster, and also an animal substance of a whitish colour. The monster weighed one pound and fourteen ounces; the substance weighed two ounces, was rather of an oval figure, and connected to the child from which it was taken, by a cord that had some faint resemblance to the umbilical. On one extremity of the substance is a small teat or protuberance, about half an inch long, and between one-fourth and one-half of an inch in diameter. Immediately by it is hair of an auburn colour, about an inch and one-fourth long. The only analogy it bears to the human is, that is covered by the epidermis.

The monster occupied part of the epigastric and the umbilical regions. It was not connected to the child from which it was taken by a cord or any visible medium. Whether a cord or other medium of connection had existed and been destroyed by putrefaction (which, from the smell of the fluid and other appearances, had commenced) could not positively be ascertained. That there must have been some medium of connection I am confirmed, as well by the universal course of nature and analogy on this subject, as by an appearance at the articulation of the cervical and dorsal vertebra, resembling faintly the divided funis.

The position of the monster in its envelope was aukward; its thighs drawn up to its abdomen, and attached to it in places, the left resting on the shoulder, and reaching as far as

the back part of the head; the right resting or pressing on the back of the right hand. The ossa femoris have perforated the flesh at the knee, and are about half an inch out. The left leg is imperfect, lies back along the thigh, to which it has grown. The right leg is also imperfect; its foot is suspended over the head. On one foot are three toes; on the other a small appearance of two. From the knees to the shoulders there is considerable perfection of form. Its sex is indistinctly marked; the indications are of the feminine. The left arm should rather be called a stump than an arm; it has on it no hand; at the end of the stump is to be discovered a nail. The right arm is large and long, it has three fingers and the thumb. The head is very imperfect, it rests upon the breast between the knees. It has neither ears nor eyes, or appearance of any substitute for either. No mouth, nor any thing that has a near resemblance to it. There is on the left side of the face, or rather that region of the head which the face should occupy, a small prominency, which contains three teeth, the canine and two incisores; they are about the size of the teeth of a child two years old. This prominency, or mouth, if it may be so called, has no aperture. On the back part of the head was hair of an auburn colour, eight or nine inches long. The body of the monster was 7 inches long, and 10 in circumference. The thighs 6 long, and 8 in circumference. The arm 5 inches long. The stump not quite 4 inches in length.

The interior of the cavity which contained the monsters, resembled the membrana decidua. This appearance was assumed; for, upon examination, there was not any vestige of membrane peculiar to the monsters discovered. Having explored this cavity, and dislodged its contents, I extended the incision through the muscular partition into the abdominal ca-

vity, and examined the viscera. They were rather pale, otherwise natural.

The little girl that those monsters were taken from, for about nine months after birth, was healthy. Her parents discovered, when she was only a month or two old, something hard within the abdomen, which continued to increase. After this time she became less healthy, but her complaints were those incident to all children. About nine months prior to her death, she began to decline, and became much emaciated; her appetite continued strong; her longings and desire for ardent spirits were great; she would become intoxicated if indulged in the free use of them; it took a considerable quantity to affect her; she drank freely an hour before her death. I believe it was the use of spirits, in part, that supported her so long. She was of the ordinary size of children at her age, had dark hair and eyes, and would have been handsome but for a gloom and melancholy that sat upon her countenance, which made her appearance peculiarly interesting. I have now in my possession the monsters, and am in hopes shall be able to preserve them. Should there be any disquisitions worthy of notice, or plausible opinions, I should be happy to receive them.

EDWARD B. GAITHER.

Dr. J. R. Coxe.

☞ See an account of something similar, by Mr. Blanche; Museum, vol. II. p. 226.—EDITOR.

A case of Fistula in Ano, in an Infant of eight Months. In a Letter from Dr. FELIX PASCALIS, *of New York, Physician for the County and City Prisons, consulting Physician of the Alms-house.*

New York, June 18, 1809.

SIR,

YOU will find important circumstances in the following case. Although simple in itself, it impressed my mind with the opinion that it might be a warning against its possible formation in an infant, and that its perfect cure should encourage a speedy instrumental process.

The male infant of Mr. Stephen Rich, of this city, was taken from his mother two months before she died with *phthisis pulmonalis*. I objected to the nurse with whom he was left, because she had her own infant as another nursling. Some time elapsed before an acceptable woman could be obtained, when this child, visibly much debilitated, began to show a considerable eruptive disease; he had also a few angry boils. A large one was observed on the internal margin of the anus, which I would have deliberately opened, had I not been strongly opposed by the woman, and requested to delay that operation.

The infant was afterwards taken away by the nurse: thus nothing better was devised than emollient poultices for the boil, and a few depleting and absorbent doses. I had strictly enjoined, likewise, that the expected breaking and discharge of matter should be well attended, and related to me for further prescription. Indeed, three weeks after, good account was given of the suppurated boil; but a new alarm having been conceived from a return of the eruption, I was called for a

second examination, when, to my great regret, I found a hole on the margin of the anus, where I had seen the boil before. My probe was run into it at the depth of an inch and one-eighth. It was, in short, a complete fistula, on the right side of the rectum. The infant was now eight months and two weeks old. I tried with an indispensable violence to introduce my little finger in the anus, to feel and follow the probe as deep as it went, and by a small seringe I ascertained that the sinus was opened in the bowel.

This case was simple, nor could I apprehend much difficulty for an operation. I judged, however, that the space on which the fistula extended was so great, comparatively to the size of the subject, that the division of some arterial branch could not be avoided, and would leave a troublesome hæmorrhage. Besides I had not, nor did I know any precedent of a similar case whatever, to remove the idea of a possible danger, on, or after the operation. I therefore thought proper to request the advice and concurrence of our worthy and skilled Dr. Valentine Seaman.

He, by all means, encouraged the operation; but he wished that, a week or ten days before, a small dose of calomel should be daily given to the infant, with the view of counteracting or destroying any specific cause of eruption, if any could exist; indeed there was none to which he had been visibly exposed, yet the appearances were exciting some suspicion.

This mode was adopted with real advantage, for it rapidly cleared off the eruption. With this preparation, care was taken to keep the bowels clean, and to prevent accumulation of matter in the fistula, by daily injections. A gentle compression was also produced over it by a small tent, to be re-

newed from time to time. These means checked the progress of the fistula, and removed every symptom of inflammation; when, in presence of Dr. Seaman, I then performed the operation in the following manner.

Having laid the infant on his belly, in the lap of a woman, the nates being gently separated by two hands, I introduced in the anus a steel-grooved canula, directing the curve end of it towards the fistula. Holding it steady with the left hand, I introduced, with the other, in the sinus, a narrow sharp-pointed knife, the end of which was made blunt with wax: this was likewise curved in the same proportion as the canula. Now, attending carefully to the equal portion of both instruments left out, and to their parallel direction, I soon felt that their ends had met together. By a gentle pressure, the sharp-pointed knife easily made its way through the wax and the rectum, to lodge itself in the groove of the canula. In the closed end of this, having fixed and well adjusted the knife, I quickly and steadily brought out both instruments, without altering in the least their respective situation. Thus the rectum and the sphincter must have been perfectly divided, on the whole length of the fistula, which was immediately proved, by introducing a probe to the bottom of it, which could be lodged in the rectum, by removing it on the opposite side, without any obstacle. Much blood had gushed out at first, which was easily and almost naturally stopped, before a tent of a proportionate size was introduced.

In less than five weeks the cure was accomplished, by the method of frequent injections, and that of a small pledget softened with cerate and kept in the anus. Nothing remained now that could indicate the least internal soreness or ulceration;

and the visible indenture of the sphincter is perfectly healed and hardened.

REMARKS.

1. The precaution of counteracting whatever specific cause of eruption could be suspected, was a very wise one, notwithstanding we could not find out the least induction to surmise it. Calomel has done good, because it is a stimulant, happily counteracting that kind of atony or debility to which infants are very subject under the influence of various slight causes.

2. This case is a warning against the neglect of a boil formed on the margin of the anus. Had this been well opened and discharged at first, no fistula would have taken place. One half of these kinds of internal ulcerations, I am satisfied, originate with boils on that part, the confined and compressed state of which causes the diffusion of matter internally and through the tender cellular texture into which the rectum is affixed.

3. A fistula, in infants, must make very rapid and alarming progress. This case was accomplished during the short space of three weeks.

An infant was offered to my inspection, in Philadelphia, during the fall of 1793, who, from an inch behind the anus, on the right side, to one inch on the perinæum, had the whole opened, corroded, and destroyed, forming a large cavity, and horrid ulcer. I have a right to suppose that it originated with a fistula.

4. It is, perhaps, deserving some attention and inquiry, why this sort of disease does not occur on the left side as often as

on the right. The fistula of this infant was on the right side, and, among the considerable number of cases I have operated upon, and many more that I have inspected, I remember of none that was on the left side, although I have seen complicated fistulas with sinuses running upwards towards the coccyx, or downwards in perinæo, towards the scrotum. In the case of Mr. William M'Kenny, jun. of this city, the fistula originated with a chicken bone, transversely lodged above the sphincter. This was found late after he had applied to medical aid. From a very large cavity on the side of the rectum, five sinuses were running outwardly; but one of them had been so much enlarged, that it had become a false anus, occasionally discharging fæces and wind. All this train of shocking ulcerations, which was perfectly cured, existed on the right side, with the exception of one sinus running downwards under the line of the *raphe*.

5. The operation in this case was so easily performed by the means of a curved canula, that I would recommend this mode in preference to introducing the finger, which might put the parts upon such a stretch as to cause distention and much irritation. I observed likewise, that simple dressings and injections, made with the greatest care, were apt to produce some excoriations. Whenever it so happened, I omitted them one or two days, and applied only an alcoholic lotion.

New York, June 18, 1809.

Extract of a Letter from Dr. PASCALIS *to the* EDITOR, *on the same Subject, dated New York, September* 3, 1809.

When I received the favour of your last letter, I was just beginning the treatment of a new case of fistula in ano, the deepest I have seen, which was seated exactly on the left side. I operated on it a few days after; but I retained some regret, lest my observations in the last piece you have been pleased to insert in the Museum, were too exclusive of the occurrences of fistula on any side of the sphincter. It remains, nevertheless, a strong opinion with me, that fistulæ in ano are more frequent on the right than on the left side, owing to the more common and usual way of lying on the right side during rest and digestion, which is thereby assisted by the weight of the aliments on the pylorus itself. If this last observation is correct, there has been something more than *accidental* in the constant occurrence with me of fistulæ on the right side.

FELIX PASCALIS.

Dr. J. R. Coxe.

Observations on the Formation of Æther, and a Proposition for obtaining it at one Operation; with Sundry Reflections. By JAMES CUTBUSH.

ACCORDING to the generally received opinion of the formation of æther, it appears, that the alcohol undergoes a decomposition, by the action of the acid employed, and that several new products are formed.

From the experiments of Mr. Cruikshank we learn, that the proportion of carbon to hydrogen in alcohol is as 8 or 9 to

1, and in æther as 5 to 1. Therefore an abstraction of a part of the carbon in alcohol must take place before æther is generated; and the manner by which that abstraction ensues is obvious, from the necessary employment of an acid.

Is it not then from this circumstance, that æther, obtained by the distillation of alcohol and nitric acid, must contain less carbon than that formed by sulphuric acid and alcohol? Is the superior volatility of nitric æther a conformation of this principle?

During the distillation of alcohol and sulphuric acid, we obtain a product of æther, alcohol, acetous acid, water, oil of wine, and a peculiar species of carburetted hydrogen gas (usually called *olefiant gas*), accompanied with sulphurous acid; of which more particular notice will be taken. The sulphurous acid, particularly, is subsequently separated by distillation from caustic potash, sulphite of potash being formed.

The second operation has received the name of rectification, as the æther is obtained nearly pure; on washing it, however, the separation of alcohol (I presume), and any portion of sulphurous acid, which passed the second process, is effected; and the æther becomes of so great a strength as to effect the solution of elastic resin. Instead of rectification by means of potash, would it not be advisable to distil the mixture of alcohol and acid from a small quantity of oxid of manganese? Would we not obtain the product at one operation, uncontaminated with sulphuric acid, were this mode to be adopted?

From the circumstance of sulphurous acid being produced, it is certainly a fact that the acid is decomposed; that this decomposition is essential to the *formation* of æther appears to

me a subject of further research. The sulphurous acid, thus produced, passes over into the recipient, and is there condensed.

The object, therefore, of the subsequent distillation from *pure kali*, as prescribed by the dispensatory, is, as we have remarked, to separate the sulphurous acid, and probably some other products; but it is absolutely necessary that the separation of this acid should be accomplished, which can only be effected by this or a similar treatment, before it is proper for exhibition as a medicine.

In the employment of oxid of manganese, or the oxid of lead, no sulphurous acid would be formed; and it appears, that were the materials to be pure and in proper proportion, there would be no occasion for a second distillation; for the quantity of oxygen given out from the oxid, would be sufficient for the conversion of the sulphurous into the sulphuric acid; but, at the same time, the sulphuric acid would not be prevented from performing its *office*. Sulphurous acid, it is well known, will assume the gaseous state; but the heat necessary for the evaporation of sulphuric acid must be at least 500° of Fahrenheit: consequently it will not distil in the temperature necessary for the distillation of æther.

The formula which has lately been adopted for the distillation of nitric acid, furnishes us with an example of the utility of oxid of manganese in the distillation of some substances, which require more oxygen; and the facility with which this substance parts with its oxygen highly recommends it for the purpose. The nitric acid, we know, is a combination of azote and oxygen, and the nitrous acid is composed of the same elements, holding nitric oxid in solution: hence, as the latter

only requires an addition of oxygen to form the nitric acid, if we distil a mixture of nitrate of potash, oxid of manganese, and sulphuric acid, we obtain the nitric acid of a perfect state of purity, which, however, is influenced by the purity of the materials*. The oxid of manganese, in this instance, acts in the same manner as it would in the distillation of sulphuric æther.

The oxygen, which the oxid gives up on the application of heat, acidifies the nitric oxid, and forms the nitric acid: consequently no nitrous acid is produced.

I am aware that there are some instances in the preparation of æther, where the employment of oxid of manganese would be improper; and then it becomes necessary to use potash and a subsequent distillation.

In preparing nitric æther, by following the process described by Dr. Black, namely, by mixing the acid and alcohol gradually, subsequent distillation from *caustic potash* is certainly advisable, to separate the redundant acid. But the doctor prepares it merely by mixture.

If oxid of manganese were employed in the process of distilling a mixture of alcohol and nitric acid, in the preparation of nitric æther, it would act in this instance by changing any portion of nitric oxid, which was formed, into nitric acid. But in the preparation of muriatic æther, which is prepared

* For an account of obtaining nitre from various sources, and the mode of working the saltpetre caves in the western country, with a variety of remarks on the formation of gun-powder, &c. I beg leave to refer the reader to a series of numbers published in the Aurora, by me, entitled, "Application of Chemistry to Arts and Manufactures."

more economically by the distillation of a mixture of muriate of soda, sulphuric acid, and alcohol, it would change the muriatic into the oxymuriatic acid.

It may not be improper to conclude, by making some observations on the subject in general.

The decomposition of alcohol by the action of acids, and the production of new compounds, furnish matter of the utmost importance in chemical inquiry; and, indeed, no chemical philosophers have so ably treated the subject as Fourcroy and Vauquelin.

To these able chemists the scientific world is indebted for the present theory of the formation of æther. They state, that the *acid remains unchanged;* but that the alcohol is converted into æther, water, and charcoal. They remark, that when the production of æther *ceases*, the sulphuric acid is decomposed, and sulphurous vapour begins to arise, which condenses in irregular streaks, or in drops.

In consequence of this circumstance, the process should be immediately stopped, or the receiver changed. We have said, that, in the distillation of sulphuric æther, we obtain sulphurous acid, acetous acid, water, and oil of wine, which is accompanied by a peculiar species of carburetted hydrogen gas; and which, on account of its property of producing an oil when mixed with oxygenized muriatic acid, the German chemists have named olefiant gas.

It is said, that, if the process is stopped before sulphurous acid is formed (adopting the theory of the French), the *whole* acid, diluted with water and mixed with charcoal, remains in

the retort. Proust has also observed, that the sulphuric acid may be obtained from the black residuum, by diluting it with water, filtering it through linen, and evaporating it till it acquires the specific gravity 1.84; then adding about a five hundredth part of nitrate of potash, and continuing the evaporation until the acid becomes perfectly colourless, and acquires the specific gravity 1.86.

If, then, the above theory be adopted, which we have no *very material* reason to doubt, what is the *modus operandi* of the process?

Does the heat possess an affinity for a compound of carbon and hydrogen, containing less carbon than alcohol, which portion the acid retains? or does the acid act in the manner previously stated? The first principle *may* be observed, and it may happen, that, after the distillation of the æther, the carbon may decompose the acid. Then we would obtain another product, which is carbonic acid; for, in consequence of a portion of oxygen being abstracted, a part of the carbon would afford carbonic acid, and the sulphuric the sulphurous acid. Another compound of carbon and hydrogen must unite with another portion of oxygen, and produce acetous acid; and the olefiant gas must arise from a solution of carbon in hydrogen, which, in all probability, has originated from the alcohol, subsequent to the production of æther. The carburetted hydrogen, by uniting with caloric, formed the olefiant gas*. The water formed may also originate from the union of oxygen from the sulphuric acid, and hydrogen from the alcohol; so, also, the union of hydrogen, carbon, and some æther, may produce the ætherial oil of wine: and thus the several pro-

* For an account of the olefiant gas, see Ann. de Chim. tom. xxi.

ducts may arise from the reciprocal action of sulphuric acid and alcohol.

Since reflecting on the above, an idea struck me, which is, whether or not the sulphuric acid acts *also* by retaining a quantity of water, which the spirit of wine usually employed contains.

This, however, I find to be the opinion of Fourcroy and Vauquelin, according to Parkinson, which work, subsequent to writing the above paragraph, I had reference to. This gentleman states, page 143 of his Memoranda Chemica, that "Fourcroy and Vauquelin attribute the formation of æther to the *attraction* of the sulphuric acid for the *water* of the alcohol." But the experiments of Mr. Cruikshank, on the proportion of carbon in alcohol and æther, make it appear, that there is a difference in the quantity of the carbonaceous principle in æther and alcohol; consequently an abstraction of carbon must take place, independent of the observation of Fourcroy and Vauquelin, before this fluid can be formed.

According to a former theory, it is stated by some chemical writers, that æther was nothing more than oxygenized alcohol; and they remarked, that it was formed of part of the oxygen of the acid united with the carbon and hydrogen of the alcohol.

Gren observes, speaking of the distillation of a mixture of *muriate of soda*, *magnesian oxid*, *alcohol*, and *sulphuric acid*, that a dulcified oxymuriatic acid is obtained, and an oily product which he called *oil of salt;* but if oxymuriate of potash and alcohol are distilled, a muriatic æther, according to Van Mons, is obtained.

It has always remained a difficult matter to prepare nitric æther; to devote a portion of our time to this subject, at a future period, will not be improper. It was my intention to have given observations on the preparation of the æthers respectively; but, in consequence of the length of this essay, I purpose performing this task on a future occasion.

An Essay on Insensible Perspiration, and the supposed Absorption and Emission of Gases by the Surface of the Human Body. By* JOSEPH KLAPP, *M. D.*

INSENSIBLE perspiration, on account of its influence on the healthy and diseased conditions of the system, is a subject of importance to the practitioner of medicine; and, as it is one of the number of intricate functions belonging to the animal body, it is eminently entitled to the attention of the physiologist.

At first sight, this inquiry may to some seem unnecessary, as it has already been ingeniously and elaborately pursued by many writers, particularly so by Sanctorius, Keil, and Cruikshank. But when it is recollected that this office of the skin is more considerably influenced by climate than by any other incidental circumstance to whose action it is exposed, it cannot seriously be supposed, that the subject has yet been exhausted. The observations and experiments of those authors may have disclosed every thing necessary to be known concerning perspiration in their respective countries, but I question whether their account of this function will be found to apply with equal propriety and accuracy in the climate of America. I am de-

* A part of this essay was read before the Medical Lyceum, at one of the meetings of last winter.

cidedly of the opinion, that, until the example which is set for us in the indefatigable labours of the great Sanctorius be followed in every climate of the world where our science has been extended, the physiologist must remain in a considerable degree ignorant of a general knowledge of perspiration.

A few authors, ancient and modern, have contended that the skin performs an analogous or subsidiary office to the pulmonary organs. They suppose, that, like the lungs, the surface of the human body *transpires* and *inhales* different kinds of gases. Had this reputed or imaginary discovery been confirmed by the experiments of other persons, it certainly would have constituted a new and highly interesting æra in the physiological science. But that some delusive circumstances or other have led these writers into a great and serious error, I conceive there can exist no doubt. My experience, which will be detailed presently, united with that of a few other writers, authorize me to assert with confidence, that gases are neither transpired nor inhaled through the surface of the human body.

The points I am particularly desirous of investigating, may be resolved into four general heads:

I. What quantity of insensible perspiration is emitted by the skin in a given space of time?

II. What are the sensible and chemical qualities of the matter of perspiration?

III. What affinity exists between the aqueous substance transpired by the skin, and that thrown out by the lungs in respiration?

IV. Are gases transpired and inhaled through the surface of the human body?

The *skin*, or investment of the human body, has usually been divided into three membranes, the cuticle, rete mucosum, and cutis vera. The conformation of the most exterior of these membranes, the scarf-skin as it is called, has been differently described by different authors. That it is composed of the lays of scales, analogous to the arrangement of the scales of fishes, is the theory of Lewenhoek. According to professor Mickel, it is a mucous substance, thrown out by the extremities of the cutaneous vessels, and hardened by the air of the atmosphere. Morgagni says, it consists of a callosity of the extreme vessels of the skin; but some later writers have discarded all these theories, and contend that the cuticle is neither lays of scales, exsiccated mucus, nor a callosity of the vessels, but is an organized membrane formed by the animal economy, in a like manner with other vital parts of the body.

Lewenhoek supposed the matter of perspiration was deposited by exhalent arteries under the cuticle, and was from thence discharged through the interstices or loose edges of the scales of the cuticle; and Mickel has asserted, that the scarf-skin is an entire membrane, and the perspirable fluid passes through it by a process of soaking, or transudation. Mr. Cruikshank, however, embraced a very different theory. He supposed the cuticle was not an entire, but a porous membrane, and that the discharges of the skin pass through certain imperceivable vessels, which run from the perspiratory vessels of the cutis, and terminate on the surface of the epidermis. It may not be improper to observe, that Mr. Cruikshank founded this opinion on what seemed to him plausible, and not on such obvious and certain grounds as could warrant its expression

with positiveness or great confidence. He relates facts, and uses much reasoning, to prove the *vitality* of the cuticle, but very candidly acknowledges, that, "after some pains, and assisted by pretty good microscopes, he was not able to discover perforations in the cuticle, or rete mucosum."

The *rete mucosum* appears, as might be supposed from its name, to be a mucous substance, bearing a resemblance to network, in consequence of certain impressions on its surface. It is situated between the epidermis, just described, and the true skin. It is the principal, if not the exclusive seat, of that great variety of complexion, which distinguishes the human inhabitants of different quarters of the world. The same dispute concerning the nature and formation of the cuticle, exists with regard to the rete mucosum. The common opinion of former anatomists is, that this membrane is not vascular; and, perhaps, inasmuch as relates to *plain* and *obvious* data, we have full as much reason to credit this ancient theory of Albinus, as we have that of Cruikshank, and some other modern authors. Mr. Baynham believed he had succeeded in injecting rete mucosum; but Mr. Cruikshank informs us, that Mr. Baynham was good enough to leave with him for some time his preparation, and that, after mature deliberation, and close inspection, he convinced himself that it was not this membrane which Mr. Baynham had injected. Malpigi, the discoverer of this "corpus mucosum et reticulare," as he calls it, asserted that he had discovered *tubes* or *vessels* in it; but succeeding anatomists, who were possessed of equal opportunities of information with himself, have attentively sought for these perforations without success. Mr. Cruikshank was of the opinion, though Mr. Baynham did not discover vascularity in the rete mucosum, his injection had given rise to the discovery of a membrane hitherto unknown to anatomists. From the time

the knowledge of Mr. Baynham's injection reached Mr. Cruikshank, it seems as if he had been spurred on to a very assiduous investigation of the subject. He says he was repeatedly successful with the skin of persons who had died of natural small-pox, in passing injecting matter into a membrane which occupies the space between the rete mucosum and the cutis. This membrane he calls the cuticula quarta, which he supposes is the particular seat of small-pox, and other eruptive diseases. He supposes this is the membrane which Mr. Baynham injected, and acknowledges, with some flattering compliments to his talents and rising fame, that he is the first discoverer of it.

Relative to the *cutis vera*, there can be no doubt as to its vascularity. It may be described to be a white, exceedingly vascular and sensible membrane. It is the organ which performs the important offices of touch or feeling, and of perspiration. Its characteristic or distinguishing properties are, its uniform whiteness, its vascularity, sensibility, and it is said to discover, under some circumstances, marks of elasticity. Different classes of porous openings on the cutis are mentioned by anatomists; the ducts of the sebaceous glands, the ducts which convey the hairs, and the perspiratory vessels.

It has been supposed, as I have before intimated, that the matter of perspiration is conveyed through certain processes or vessels, which run through the cuticle and rete mucosum, and are attached to, or inserted in, the openings of the exhalent arteries on the surface of the true skin. But this opinion is a theory with Mr. Cruikshank which can claim no other support or authority, than what it has derived from his ingenuity. It may possibly be, that the rete mucosum and cuticle are vital membranes, and possessed of both exhaling and absorbing

vessels, but anatomists as yet have fallen far short in their exertions of proving either the one or the other. At present, I sincerely believe the fact is, though many theories have been proposed to explain the manner the perspirable fluid is taken up from the extremities of the exhalent vessels in the cutis, and conveyed to the external cuticular surface, still the question is an undecided point. It is impossible, in the present state of our knowledge, to conclude with any degree of certainty which of the above opinions are true, or that any one of them is so. The true explanation is perhaps reserved for some future period, when the anatomical study may be pursued under auspices more conducive to its progress.

Having made these preliminary remarks, I shall next undertake the prosecution of the first object of the present inquiry:

I. What quantity of insensible perspiration is emitted by the skin in a given space of time?

Sanctorius and Keil calculated the quantity in four and twenty hours, by weighing the body before and after sensible evacuations in the morning. Though I have never possessed the convenience of the balance used by those experimentalists, yet I must confess, making a due allowance for the halitus of the lungs, it seems to be calculated to afford a more accurate estimation of the degree of the discharge, than any other method with which I am acquainted; not excepting even the one I have at present adopted. It was with regret I found it necessary to relinquish a previous intention of making experiments on a large scale, and to pursue what I must term the less certain method of Mr. Cruikshank. Weighing the actual vapour of perspiration, collected from a small portion of the whole surface of the body, may have seemed to that gentleman

as the best way of investigating the subject, but though I have no doubt it will serve to throw much light on the doctrine of perspiration, yet I never can recommend it so highly as he has done. I cannot avoid being sensible in my own mind, that *the degree of perspiration of the same parts, varies at different times; and also that the quantity of perspirable fluid emitted by the different parts of the whole cuticular surface at the same time, is considerably different.*

EXPERIMENT I.

Fahrenheit's Thermometer 70°.

Two hours *after dining*, my hand was introduced into a wide-mouthed bottle, which would contain about half a gallon. To intercept all communication between the air without and that within the bottle, a piece of varnished silk was closely fastened around the mouth of the bottle, and, on introducing my hand, the other end of the silk was, with a piece of wide tape, fastened to my wrist. To answer this purpose, Mr. Cruikshank used part of a bladder. But having ascertained that a moistened bladder, in a warm temperature of the air, will, in a short time, form fixed air, which might give rise to a deception, I was of the opinion it would be improper to use it. In this way, perhaps, the fixed air, which Mr. Cruikshank believed he found in the bottle after withdrawing his foot, was produced; at any rate I shall presently relate many experiments, which show that in all probability it could not have been discharged by the skin, as he imagined. The statement, or rather the opinion of Mr. Cruikshank on this point should be cautiously received, as his experiments were afterwards frequently repeated, by that veteran in philosophy, Dr. Priestley, without on any occasion discovering the least fixed air. I should be very sorry to be understood to impeach the veracity

of so respectable an author as Mr. Cruikshank was; and, in order that so unjust an impression may not be taken, I now declare, that my only meaning or wish is to suggest that he may have been mistaken, in imputing the transpiration of fixed air to the skin, and in fact he may have been deceived with regard to the fixed air altogether.

In the course of a few minutes after placing my hand in the bottle, its transparency was effaced by the abundance of the vapours of perspiration; and in a few minutes more the internal surface of the bottle was covered with small drops of these vapours, condensed into a liquid. To promote the running together of these drops, and their descent into the bottom of the bottle, I had recourse to the means used by Mr. Cruikshank. A linen cloth, dipped in cold water, was wrapped equably around the bottle, with the view of condensing the vapours within, by producing a process of gentle evaporation from the external surface of the bottle. The application of the wet cloth accomplished the desired effect; for the vapours of perspiration were pretty soon converted into a liquid.

At the expiration of an hour, my hand was withdrawn, and, to all appearance, there was collected a large tea-spoonful of transparent liquid. My first desire was to ascertain whether fixed air had been thrown out by my hand into the air contained by the bottle. As I have mentioned, Mr. Cruikshank supposed such was the case in his experiments, or that something, which he believed might be phlogiston, was discharged by the skin, and, by uniting with a portion of atmospheric air in the bottle, formed fixed air. A lighted taper was directly on withdrawing my hand introduced into the bottle; but the flame was not thereby in the least diminished. About a half ounce measure of the air was next passed through lime water, and, though it

was agitated in the water, the volume of the air was not diminished, nor were white clouds produced in the lime water. I think I may therefore say, carbonic acid gas was not thrown out by my hand.

On weighing the liquid collected in the bottom of the bottle, I found it was equal in weight to twenty-seven grains, according to a very accurate pair of scales. Therefore, proceeding to calculate agreeably to Mr. Cruikshank's rule, it is proper to say, if the surface of my hand is to the surface of the whole body as one is to sixty, and if the perspiration of all parts be equal, then in twenty-four hours six pounds and nine ounces of insensible perspiration were emitted by the skin.

EXPERIMENT II.

Fahrenheit's Thermometer 60°.

In the morning, one hour *before breakfast*, my hand was introduced into the bottle, and confined with the varnished silk as before described. The matter of perspiration collected on the surface of the bottle in a very sparing and tardy manner. When one hour had expired, my hand was withdrawn; and although the evaporation of water from the external surface of the bottle was used some time previous to the discontinuing of the experiment, yet the quantity of the condensed perspiration collected did not exceed a few drops. To ascertain whether any fixed air had been emitted by the skin, a lighted taper was introduced into the bottle, but without occasioning any alteration or diminution of its flame. Some of the air of the bottle was next examined over lime water, but no discoloration of this chemical test was perceived to occur. The few drops of condensed insensible perspiration at the bottom of the bottle were poured into the scale of a balance which contained a bit of

sponge, the exact weight of which had been previously ascertained. With this piece of sponge the small quantity of perspiration still adhering to the inside of the bottle was imbibed with much care, when it was again returned to the scale. After deducting the weight of the sponge, the quantity of insensible perspiration collected was only six grains. It may therefore be said, if the surface of the hand is to the whole surface of the body as one is to sixty, and if the perspiration of all parts be equal, then, during four and twenty hours, in a moderate temperature of the atmosphere, while the excitement of the system is languid, and the stomach empty, the quantity of insensible perspiration in twenty-four hours is only eighteen ounces.

This experiment, compared with the others, will serve to show, that the discharge by the cutaneous passages is different at different hours of the day, less fasting than after eating; or, in other words, it will show that the discharge varies in degree as certain causes more or less concur to influence it.

EXPERIMENT III.

Fahrenheit's Thermometer 80°.

Two hours *after breakfasting*, my hand was again introduced into the bottle, and embraced with the varnished silk as in the other experiments.

The vapours of insensible perspiration, as usual, in a short time began to collect on the surface of the bottle, and, on being resolved into a liquid state, gradually passed down into its bottom. The experiment was continued one hour, and in ten or twelve minutes previous to its conclusion, the wet cloth was applied equably around the bottle. On withdrawing my hand,

I proceeded to weigh the condensed perspirable fluid, not deeming it necessary to bestow any additional attention to the chemical quality of the air in the bottle. The quantity collected proved to be fourteen grains; therefore, with the provisions before stated concerning the cuticular surface, and the perspiration of different parts, it is proper to say, in twenty-four hours forty-two ounces of insensible perspiration were thrown out by the cutaneous passages.

EXPERIMENT IV.

Fahrenheit's Thermometer 75°.

One hour *previous to dining*, I executed the present experiment, with the same apparatus before used. My intention in selecting different times of the day for the making of the present experiments, is to endeavour to discover what influence the state of the stomach, as to emptiness, and its containing aliment, has on the extent of the discharge by the skin. When one hour had expired, I withdrew my hand from the bottle, and poured out the condensed perspirable fluid into the scale. Its weight was twelve grains. If, then, the perspiration of all parts be equal, and the surface of my hand is to the surface of my whole body as one is to sixty, in twenty-four hours I lost thirty-six ounces of insensible perspiration by the skin.

EXPERIMENT V.

Fahrenheit's Thermometer 90°.

Mr. R. C., at my request, *immediately after dining*, introduced his hand into the bottle, and intercepted the communication of the external air in the usual manner. The wet linen cloth was applied to condense the vapours. The hand was removed in one hour; and, on pouring the perspirable liquid

into the scale, it appeared twenty-two grains had been collected, which, multiplied by sixty, and then by twenty-four, for the whole cuticular surface, and the given time, would make the extent of this discharge, allowing it to be the same at all times, and equal from every part, sixty-six ounces.

By Sanctorius, the average quantity of insensible perspiration discharged by the skin *in Italy*, was estimated at fifty ounces in twenty-four hours; by Keil, of Northampton, *England*, it was calculated to be thirty-one ounces; and Mr. Cruikshank, of the same country, has made it nearly three times the latter quantity. If the method of experimenting which I have adopted be just, and if I have made no mistakes in its execution, it will be easy to perceive the degree of insensible perspiration by the skin in the climate of *the United States*. From much attentive observation, and many experiments on the subject, made in different states of the atmosphere, and in different months of the year, and comprizing both what is contained in this essay, and what I have not conceived necessary to mention, I am induced to believe, that the average quantity of insensible perspiration, in this country, is somewhere about forty-one or forty-two ounces in twenty-four hours. It must not, however, be forgotten, that the proverb, "circumstances alter cases," which is very commonly mentioned in speaking of small matters, is perfectly applicable to this office of the skin. Age, constitution, temperature of the air, diet, exercise, and a multiplicity of other causes, have no inconsiderable influence over the insensible perspiration.

II. What are the sensible and chemical qualities of the matter of perspiration?

The following appear to be its principal sensible qualities: it is limpid, tasteless, and in liquidity it is about the same with water. It is somewhat lighter than water, as was ascertained by the next experiment.

EXPERIMENT VI.

Of condensed insensible perspiration, ten drops were placed in one scale of a very delicate balance, and an equal number of drops of water were placed in the other scale. The drops of both liquids were as nearly of the same size as could well be obtained. They were dropped out of the same vial, and by the same hand. On raising the beam, the scale which contained the water preponderated. The difference between the weight of the two fluids proved to be three grains.

My convenience has not been such as would enable me to say much concerning the chemical properties of the insensible perspiration. I have however experimented so far, as to ascertain that it is possessed of neither *acid*, *alkaline*, nor *astringent qualities*.

EXPERIMENT VII.

Some of the tincture of blue cabbage was poured into a vessel, which contained a small, but sufficient quantity of condensed perspiration, without producing either a red or a green colour. I therefore concluded it did not contain either an acid or an alkaline property.

EXPERIMENT VIII.

Some of the solution of oxy-sulphat of iron was added to a small quantity of condensed insensible perspiration, but without occasioning a black colour, or the least precipitation of the metal. From which circumstance it was inferred, that the matter of insensible perspiration is not possessed of an astringent quality.

III. What affinity exists between the aqueous substance transpired by the skin, and that thrown out by the lungs in respiration?

To investigate this subject, it was of course necessary to collect the aqueous matter thrown out by the lungs in expiration; and as the *quantity* of this discharge could be at the same time so easily ascertained, I resolved to include this additional object in my experiments. The method of these experiments was simply this: one end of the silk investment before used was fastened to the neck of a half gallon bottle, and in the other end was included a tin tube, about a foot in length. An apparatus of this description would enable me to empty my lungs, through the tube, into the bottle, in order to condense and to collect the halitus, and at the same time afforded me the advantage of not being incommoded by the non-respirable gases, thrown out by the pulmonary organs.

EXPERIMENT IX.

At three o'clock, P. M. I commenced collecting the aqueous discharge of the lungs, by expiring through the tube into the bottle, and inspiring the air from the atmosphere, which was

necessary for the important purposes of respiration. In a short time the condensed halitus began to run down the internal surface of the bottle, and to collect in its bottom. At the expiration of half an hour the process was discontinued; when, according to my scales, I had collected one drachm of liquid from the air thrown out by the lungs.

The whole of this discharge, then, for twenty-four hours, would be six ounces, allowing it to be the same in degree at all times. I next proceeded to ascertain what analogy existed between the sensible and chemical qualities of this discharge, and that of the skin. In appearance, liquidity, and taste, I was not able to discover any difference between them. Like the vapours of insensible perspiration, the halitus of the lungs, by condensation, forms a *thin*, *tasteless*, and *limpid liquid.* Lime water added to it occasioned no precipitation; nor does it change the colour of the tincture of blue cabbage. No gallat of iron was formed, on adding to it some of the solution of the oxy-sulphat of iron. This experiment was afterwards frequently repeated, with the same result, excepting what relates to the quantity of the aqueous matter emitted, and even in this respect the variation was so trifling, as scarcely to deserve mentioning. One of these variations, perhaps, it may be proper to relate.

EXPERIMENT X.

At half past 12 o'clock, P. M. I began this experiment; the same apparatus was used as described in the last. The halitus from the lungs, as usual, in a few minutes presented itself in a liquid state on the internal surface of the bottle. The process was continued precisely half an hour. The condensed liquid was poured into the scale of the balance; and its weight was ascertained to be sixty-nine grains. The addition of lime

water, tincture of blue cabbage, and oxy-sulphat of iron, to the condensed liquid, was followed by the same results as mentioned in the ninth experiment. The liquid was *limpid*, *tasteless*, and possessed of neither *acid*, *alkaline*, nor *astringent* properties.

From these experiments I have concluded, that there exists no difference between the liquid afforded by the halitus of the lungs, and that which is formed out of the vapours of insensible perspiration. Perhaps the process by which they are formed and discharged from the system, is the same. The two following reasons incline me to suspect that the halitus of respiration is formed by a process of secretion in the lungs, and not by a chymical combination, as is very commonly believed. 1st, The natural temperature of the lungs seems insufficient to excite the affinity of combination between the constituent gases of the halitus. 2d, The vapours of insensible perspiration, which afford by condensation the same substance with the halitus of the pulmonary organs, are certainly the product of a vital process of secretion in the skin; and as the substance or liquid is formed by glandular or organic action in the one instance, in all probability it is in the second formed in the same manner.

To be continued.